SCIENCE

VOL. 80 FRIDAY, AUGUST 31, 1934 No. 2070

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

New York City: Grand Central Terminal

Lancaster, Pa. Garrison, N. Y.

Annual Subscription, $6.00 Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

ARE THERE PERIODS IN AMERICAN BUSINESS ACTIVITY?*

By Professor EDWIN B. WILSON

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WHEN I was spending a pleasant and instructive semester here in Berkeley five years ago, one of the talks I gave was on rainfall in Boston from the forty-year record of the Blue Hill Observatory of Harvard University. I showed that dry months had not followed dry months any more or any less than heads follow heads in tossing a coin. This may not seem a very satisfactory result of a considerable statistical study, but it is one of the especial functions of the statistician to discuss precisely this point, *viz.*, as to whether the behavior of phenomena is no more than might be expected of a chance series. We must remember that chance refers to the future, to the unknown. If we toss a fair coin fairly there is before each throw an equal chance of heads or tails. After the throw has been made and the result has been observed there is not chance but certainty as to what did happen. There are all too many persons who have a feeling that if throws of a coin have been running to heads, they will continue to run to heads and altogether too many who contrariwise have the feeling that if the throws have been running to heads they must tend to change and run to tails to even the series up. Neither of these feelings is correct; the chance of the next throw coming heads or tails is even and independent of the results of previous throws—always assuming that we are dealing with a fair coin

* Address before a general session of the American Association for the Advancement of Science, given at the University of California, June 21, 1934.

fairly thrown. If, therefore, one can demonstrate that the runs of wet or dry months in the vicinity of Boston are as runs of heads or tails at coin throwing one has learned something rather definite which will tend to show that he can not predict the unknown result in the next month from the known results of previous months. Of course the statistician always hopes that he will find that the phenomenon does not behave entirely as though it were a chance series so that he may have some lawful residue in the behavior of the phenomenon which will enable him to forecast successfully to some extent; he hopes in other words to obtain some control over knowledge as to what the future will bring forth, even though he may be unable to prevent untoward or to facilitate favorable happenings. And of course he hopes that he may further learn enough to enable him to exercise an actual control over future happenings.

I should like here to interject the remark that our actual control over the future is much less than many persons believe, even in the field of natural science. We have as yet no means of controlling the weather, we can not control the tides, we can not control eclipses, despite the perfection of astronomical forecasts based on the analysis of periods. In many fields the knowledge of the future is as yet serviceable to us in the way of control only to the extent of permitting us to control our own conduct so as to take advantage of or so as to avoid the disadvantages incident to the phenomena which we forecast. The important rôle of self-control, of self-adjustment to what will, or even to what may, happen should not be overlooked; it is likely to be overlooked and I think there are many evidences that it has been overlooked.

The tides are predicted to a considerable degree of accuracy by their analysis into periodic elements. If we could equally well analyze the tides of business activity into periodic elements we might be able to foretell the future well enough to enable us better to adjust ourselves to the coming fluctuations, even if we could do nothing to influence them, as we can not in the case of the tides. Of course, although adjustments to the ebb and flow of the marine tides does not in any way affect those tides, it is probable that any knowledge of the future ebb and flow of business which would be adequate to be of substantial avail as an aid in our adjustment to it would as a matter of fact modify the business tide.[1] There may be this real complication in the social forecasting, *viz.*, that possibly a knowledge of the future if we could gain it from the study of the past would so modify that future that we could not hope to forecast it without taking into account the degree to which such knowledge as we had of it would influence its course—all of which sounds like an Irish bull, but only means that social phenomena may have to be treated by the as yet poorly developed methods of differentio-integral equations.

That of which I wish to speak to-night is a statistical analysis we have made[2] to ascertain whether there are periods in American business activity, as there certainly are in the tides. The method used is that of periodogram analysis developed 30 or 40 years ago by Sir Arthur Schuster to discuss the problem of periods in meteorological or astronomical phenomena such as those of sunspots or terrestrial magnetic storms. One may show that if there were in a long series of data a period which was not disturbed by interference with other nearby periods or by fortuitous fluctuations, one could by a certain mathematical calculation compute from the data a curve called the periodogram with a pronounced peak indicative of that period. The converse is unfortunately not true, namely, if by that computation one finds a periodogram with a peak there may be in the phenomenon no corresponding simple period of the tidal type. The difficulty is that the theory of the periodogram presupposes a long series of data, long enough to permit the disturbances due to accidental fluctuations to balance each other out, long enough to enable nearby periods to be distinctly separated from one another, long enough to permit the subdivision of the series to test the subdivisions for periods—and in a practical case the series though long may not be long enough. As a matter of fact we have shown that the longest index of business activity which we have (that of Leonard Ayres[3] running by months back to 1790) is too short to give satisfactory results. When the periodogram for this index of business activity was constructed it appeared that there were peaks in the curve suggestive of periods in business, but that these peaks were not in the same places for the whole data and for different 70-year sections of the data, indicating that at best there might be no sufficient definiteness and constancy of the periods to make them useful for forecasting.[4]

[1] I recall that when a prominent New York banker was introducing Leonard Ayres some years ago he commented on the remarkable way Ayres had forecast the top of the bond market several months in advance, but added that if his bank had believed that forecast the top would have come earlier because of the effect of their selling of bonds in anticipation of the culmination of the rise.

[2] The Periodogram of American Business Activity, *Quarterly Journal of Economics*, May, 1934, pp. 375–417, prepared with major assistance of Miss Margaret Hilferty and Miss Jane Worcester. The editor of the journal has kindly permitted the use of such material from the paper as I need for my present purposes.

[3] The index was published by the Cleveland Trust Company under date of August, 1931.

[4] For the details it is necessary to refer to our complete article; intricate matters of the sort here under consideration can at best merely be adumbrated in a summary.

The statistician, however, has always one primary question to solve, as has been mentioned, namely, whether the phenomenon might after all be irregular as though due to chance. Now one of the real contributions of Sir Arthur Schuster in his famous papers was a test to determine whether the fluctuations in a series were or were not chance fluctuations. To apply the test it is necessary to have the periodogram. If then one finds the result that the oscillations of the series are fluctuations, one should refrain from interpreting the peaks as indications of true periodicities. When this test of Schuster's was applied, we found that it showed that the oscillations of Ayres' Index of American Business Activity were essentially fortuitous. Schuster's test has, however, been called in question by other investigations of fluctuating phenomena and has been replaced by one which when applied to our case would indicate that the fluctuations were not fortuitous but probably contained periods. Thus the inference we should draw must depend on whether we accept Schuster's test or that of his critics.

The question, therefore, becomes one of decision between two scientific propositions. How is such a decision made? When the matter is essentially one of mathematics or of logic as in the case of these tests, it may be possible to make a decision between two opposing propositions by a critical logical or mathematical analysis of the proof of the propositions. Theoretically, it should always be possible to make the decision in this way—either the one or the other party has obtained an incorrect test, the reasoning of one or the other or of both has been in some respect incorrect. Practically, however, it is often impossible to detect the error in an erroneous line of argument and appeal has to be made to experiment as it has always to be made in those cases when the opposing criteria are not strictly mathematical or logical. We therefore proceeded experimentally. We noted that during the 140 years from 1790 to 1929, the index had 42 complete swings from normal to high to normal to low and back to normal, or, if you prefer, 42 swings from top to top—it makes little difference at what phase of the oscillation one considers that the fluctuation begins. It was found that the duration of a complete swing was on the average about 40 months but that the duration varied from a few months to eight years (Fig. 1, left). It was found, also, that the magnitude of the swing averaged 20 per cent., but varied from practically nothing to about 45 per cent. (Fig. 1, right). (In the current swing, which is not yet complete, the oscillation has been greater than 45 per cent.; what the duration may prove to be no one knows, but it has not yet reached that of previous long swings.) We then took these

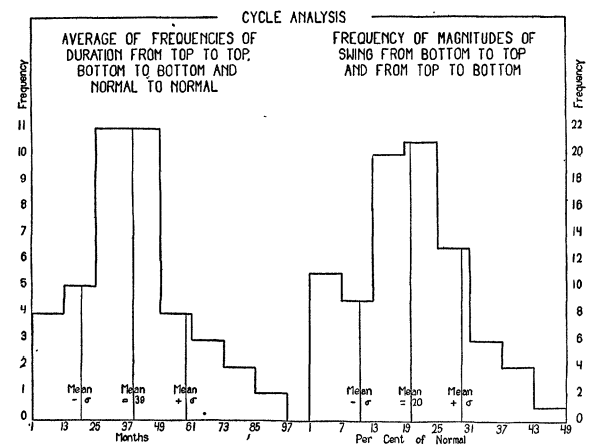

FIG. 1. The histograms or frequency distributions of (1) durations of complete swings (left, with duration in months as abscissas and frequency as ordinates) and of (2) magnitude of swings (right, with magnitude as abscissas and frequencies as ordinates, the magnitudes being figured from top to bottom and from bottom to top so that there are two magnitudes for each complete swing). The horizontal scales have been chosen so that the standard deviations are the same. The frequency distributions are not too dissimilar, but the correlation between the individual durations and magnitudes is not large.

42 swings as units, drew them serially by lot and pieced them together into a new artificial index to which we applied an analysis similar to that which we had applied to Ayres' index and with very similar results; the new periodograms looked like the old ones (Fig. 2, a, b), and like them they showed no signifi-

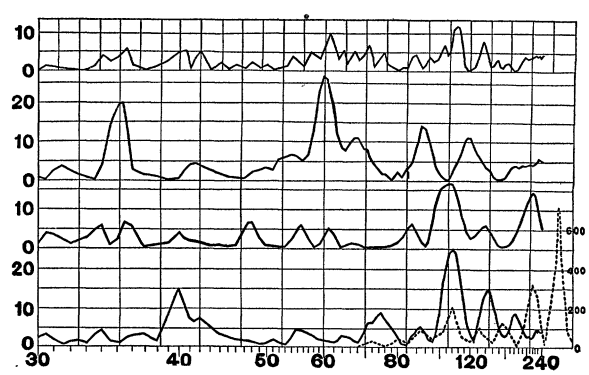

FIG. 2a. Periodograms of Ayres's Index: Top, for the whole series, 1790–1929; second, for 1790–1859; third, for 1825–1894; bottom, for 1860–1929. (The dotted curve at the bottom may be ignored.)

cant periods by Schuster's test but significant periods by the modification which has been proposed for testing significance. As it is difficult to believe that a random rearrangement of the swings could have

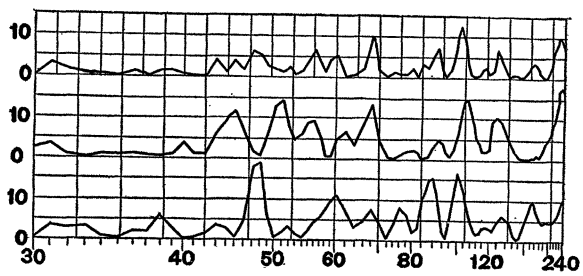

FIG. 2b. Periodograms of the series obtained from the rearrangement of the complete swings by lot: Top, for the whole series—to compare with the top of Fig. 2a; second, for the first half of the new series; bottom, for the last half.

periods we infer that the modified test is invalid and as the periodograms behave similarly with respect to the tests we infer that there is no more and no less periodicity in Ayres' Index of American Business Activity than there is in a random rearrangement of its component individual full swings.

Such an inference would imply that we could hardly expect to forecast as we do in the case of the tides by resolving the index into periodic terms and using those terms for purposes of extrapolation. But we could try the method, unpromising though it seemed, and this was done. Of course one must not depend on any single instance of forecasting, because one might have an accidental agreement or disagreement between forecast and realization which would be unduly favorable or unfavorable. We had three periodograms for three different 70-year sections of the data. From the indications of periods such as they were in the analysis of the data from 1790 to 1859 we constructed an expression made up of periodic terms from which we could determine how well the original series was represented and how well the expression forecasted. Taking the matter by decades the representation was pretty good, but the forecast was worse than useless. Similar indications were applied to the 70-year sections 1825–1894 using both backward and forward extrapolations and to the 70-year section 1860–1929 using a backward extrapolation. On the whole the forecasts were of average merit just about zero. Moreover, we did not get from the work any expression which would at all satisfactorily forecast the period 1930 to date. This did but confirm our inference that there were no effective periods in American business activity.

Such a conclusion is not dissimilar, so far as it goes, to that recently stated by Alter[5] as a result of long-continued analysis of data on English rainfall, namely (1) periodic terms do not exist, (2) nothing has been found to give long-range predictions a commercial value. This does not mean that other methods of analysis which may sometime be discovered must also fail to give long-range predictions of value for English rainfall or for American business activity. For example, some time ago Mr. Moe, of the Guggenheim Foundation, sent a young Argentinian Mr. Mata to see me. Mata believes that business activity is a correlate of solar activity and with a good deal of ingenuity he has established some rather high correlations between past records of these two types of fluctuating phenomena. I have said that we wish to resolve phenomena into periods to help us predict, but if we have two phenomena, say B for business activity and S for solar activity, and if the pattern of the fluctuations of the two are very much alike, but B follows S in time with a certain lag, then clearly we could predict B from S by the extent of that lag.

Now by whatever means one produces predictions, whether by resolution into periods or by correlation with a phenomenon which runs ahead of the one predicted, or by the exercise of general judgment, or by crystal gazing or incantation, there is one thing which the statistician may do—he may take the predictions themselves, provided they are not so oracular that they mean nothing definite, and he may seek to determine whether the predictions have in fact predicted any better than by chance. When I was president of the American Statistical Association, I was successful in persuading S. L. Andrew and H. M. Flinn, of the American Telephone and Telegraph Company, to prepare on rather short notice a paper appraising economic forecasts. The results of their statistical examination of the correspondence between forecast and fulfilment for a number of forecasting services showed that during the period 1924–1929 there had been a considerable degree of success in professional forecasts of general business, of commodity prices, of money rates, of automobile production and of construction, but no success at all in forecasting stock prices; indeed, stock price forecasts were more often wrong than right[6] (Fig. 3). The period 1924–1929 was short and perhaps an easy one in which to forecast general business or commodity prices or money rates, and possibly a more than ordinarily difficult one in which to forecast stock prices. Messrs. Andrew and Flinn did themselves feel that a more careful examination of a longer record of forecasting was desirable before one could come to sound statistical conclusions on the matter of forecasting. So far as forecasts of stock prices are concerned the matter has recently been examined intensively by Alfred Cowles 3rd in the official journal of the Econometric

[5] *Monthly Weather Review*, 61: pp. 345–350, December, 1933.

[6] *Jour. Amer. Statist. Assoc.*, 25: 169A, pp. 36–41, March, 1930. The figure is reproduced by kind permission of Mr. Andrew and of the editor of the journal.

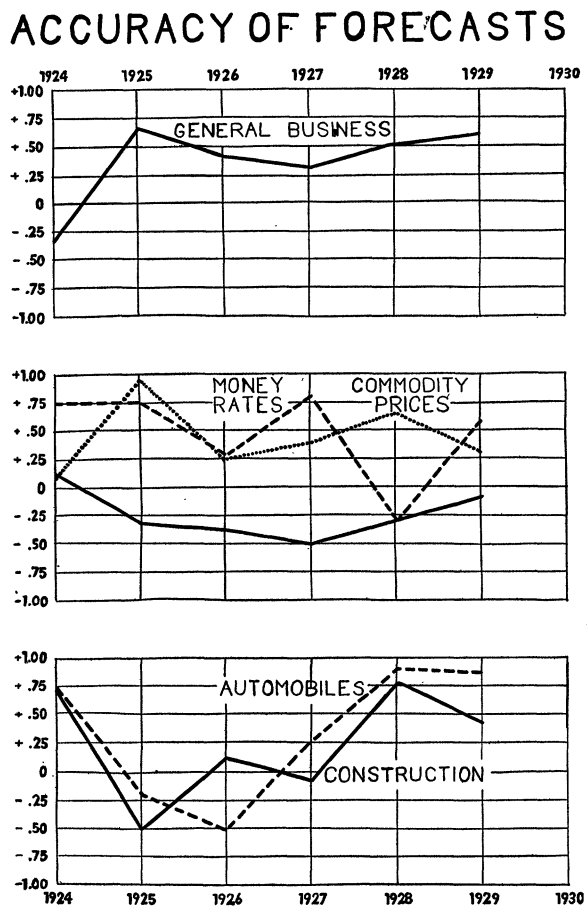

FIG. 3. From Andrew and Flinn. Accuracy of forecast. On the scale 1.00 represents complete accuracy and −1.00 complete incorrectness.

Society, which is meeting here with us.[7] His conclusion is that the forecasts are not so good as random guesswork, thus confirming the conclusion of Andrew and Flinn (Fig. 4). Indeed, he finds that the best forecasts are no better than should be expected in a series of forecasts by guesswork and that the worst forecasts are decidedly worse than those in a set of random records.

Just how bad forecasts of stock prices may be at times can perhaps best be seen by consulting the record of Irving Fisher. Here is a person of the very highest rating as a theoretical economist and of no mean competency in statistical technique, who at about the time I was here five years ago was running a syndicated financial column in the newspapers. He was then one of the New Era economists and there are some indications that he is now a New Deal economist. If you can be amused by tragedy, you would take pleasure in skimming through the compilation of

[7] *Econometrica*, 1: pp. 309–324, 1933. Mr. Cowles has kindly given me permission to reproduce his Figure 1.

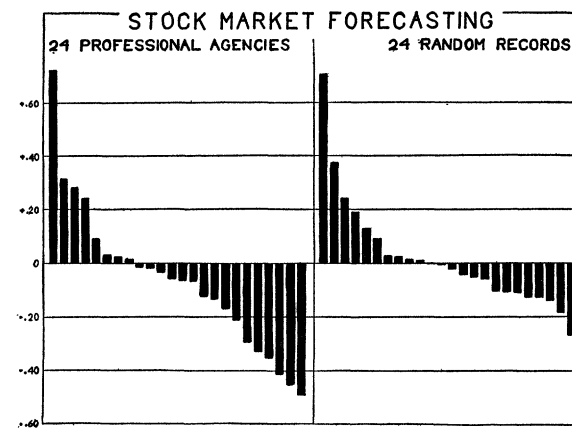

FIG. 4. Comparison of the percentage accuracy in stock market forecasting of 24 Professional Agencies and 24 Random Records (after Cowles). Note that the best records of the forecasters are no better and that the worst records are worse than for the chance series.

Stewart Angly entitled "Oh Yeah?" published by the Viking Press in 1932. But I shall not quote Fisher or others from this compilation of statements appearing in the press. It is not necessary. Fisher wrote a book entitled the "Stock Market Crash and After," published by Macmillan in 1930. The main thesis of this book appears to me to be that the New Era is still with us, that the crash in the autumn of 1929 was to be sure more serious than he had expected, but that it was to a large extent accidental, and that the new high 1926–1929 plateau of stock prices of which he had spoken would not be broken and that we were heading toward a mild boom. As a matter of fact we all know now that nothing could have been wider of the mark. We were facing a disastrous world depression, and stock prices were not on the new high plateau but on a toboggan destined to reach quotations approximately as low as any in the last 40 years (Fig. 5). Indeed, what Fisher meant by using the word plateau in the phrase "the 1926–1929 plateau of stock prices" is very difficult to imagine; the charted course of the market from 1897–1929 should seem to give the impression that the 1926–1929 period constituted a steep mountain side rather than a tableland—whether the abrupt climb was a transition from an old low level to a new markedly higher level or whether it was merely one side of a sierra which when crossed would bring us back to essentially the old level was the problem of the forecaster.

In his book Fisher remarks that "hindsight" is always clearer than foresight. I fail to see that his hindsight as shown in the book is any clearer than his foresight as expressed in his earlier newspaper items. Quite the contrary, it seems to me that the explanations offered by this "clearer" hindsight are largely

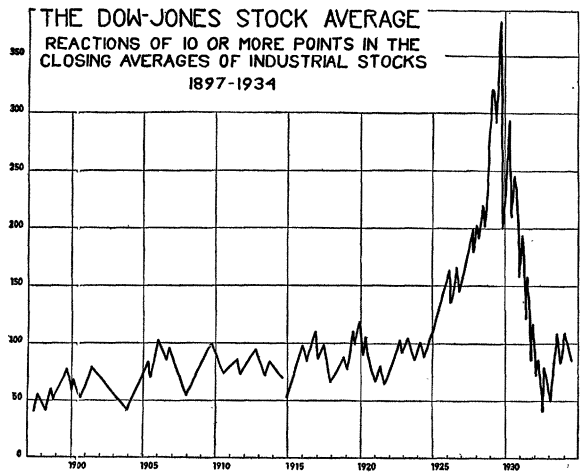

FIG. 5. The Dow-Jones Industrial Average 1897–1934. Note that this average returned in 1932 to approximately the lowest figures of 1897–1905 and in 1933 to approximately the high figures of 1905–1924.

puerilities of the sort one hears around tickers in brokerage offices from disappointed speculators who have overstayed their market and are trying to "kid" themselves that the turn has not really come instead of seeing clearly what this situation is and selling out with the losses they have rather than staying on to run up greater losses.[8]

Let me quote from a paper by Carl Snyder[9] read in December, 1929. Speaking of brokers' loans and the pyramiding of credit he said: "My knowledge of financial history is not exhaustive, but so far as I have been able to discover there appears to have been nothing quite like it since the days of John Law and the Mississippi Bubble, and the South Sea Bubble in England. And apparently the process was almost identically the same. In the case of John Law he actually printed the money . . . with which the securities he issued were bought. . . . With a slight change in the counters, much the same thing appears to have characterized the last phase of our most spectacular stock boom. Our Wall Street financiers seemed to see no limits to which the fiction of 'new values' could be carried. What repercussion the collapse will have upon the industry and the employment of the country remains to be seen."

This is not economic or stock market forecasting, but merely a recognition that we do not know for certain what the future has in store, with the suggestion, based on analogy with a considerable number of past events of a somewhat similar nature, that the future may not be all rosy. It seems a very different note from Fisher's "Stock Market Crash and After."

In view of what I have said about the difficulty if not the impossibility of forecasting business activity or stock prices with any degree of assurance, you will realize that I could not blame Fisher for his wrong forecasts because they were wrong. True, I do not think a distinguished economist should make such wrong forecasts; but that is because I think he should know and should have known full well by 1929 that there is little reason to believe that sound forecasts can be made and should therefore restrain himself, as most good economists do, from spreading such forecasts in the press. So far as I can see, Fisher in 1929 missed foreseeing in any respect the greatest economic disturbance of this country, possibly the greatest of a century. He "explained away" the stock market crash; he did not see what it meant. It is difficult to see how anybody could have been much more wrong. Neither statistics nor economic theory saved him, indeed it is difficult to avoid the impression that his statistics and his economic theory did but serve to blind him the more completely to what was impending. If he were so wrong, then, what is the chance that he is right now, when the New Era economist has become a New Deal economist and when he who was loudly assuring us of the permanently high level of stock prices has become as loud an advocate of reflation? And what but discouragement can this kind of behavior be to social science or to those social scientists who believe that valid science, that science which is real science, whether in the social field or any other, must have some relation to what happens somewhere else than in the mind of him who elaborates it?

I will not attempt to sketch the economic reasons for any of our errors in forecasting, but I may perhaps allude to an error due to the limitations of limited statistical analyses. We have really numerous and comparable data available only over relatively short periods of time—in some lines only since the great war. Now the major economic swings may be of very long duration. Take, for example, the history of prices (Fig. 6). Different price indices will give results differing in detail, but by and large the facts are that prices were low between 1750 and 1770, were high during the revolutionary war (say around 1780), continued fairly high during the Napoleonic period with a peak in our war of 1812, were low from 1820 to 1860, high during the civil war, low from 1880 to 1910, and high during the recent war.

[8] I would commend the article of G. P. Watkins, *Jour. Amer. Statist. Assoc.*, 25: 169A, pp. 18–22, March, 1930, and especially his reference to the dangers of heavily discounting future earnings in the prices paid for stocks to any who have read Fisher's Chaps. V–VI, and to all who may become involved in some future market in which stocks are paying good dividends and yet are selling to yield only 2 per cent. to 3 per cent. in a time of reasonable prosperity and reasonably high money rates.

[9] *Jour. Amer. Statist. Assoc.*, 25: 169A, pp. 88–92, March, 1930.

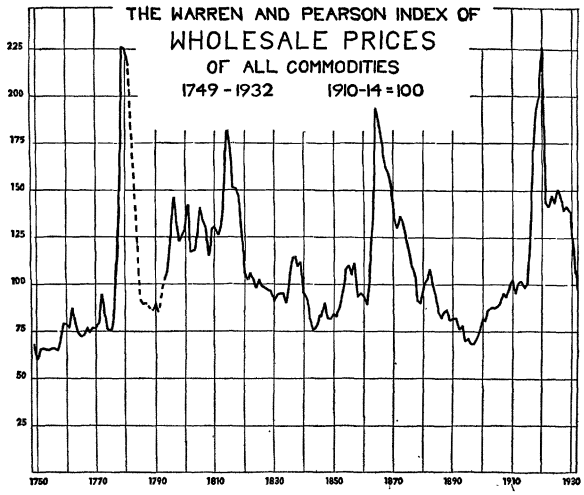

FIG. 6. Plot of the Warren and Pearson Index of Wholesale Prices, from figures in their book prices (John Wiley and Sons, 1933) on pp. 11–13.

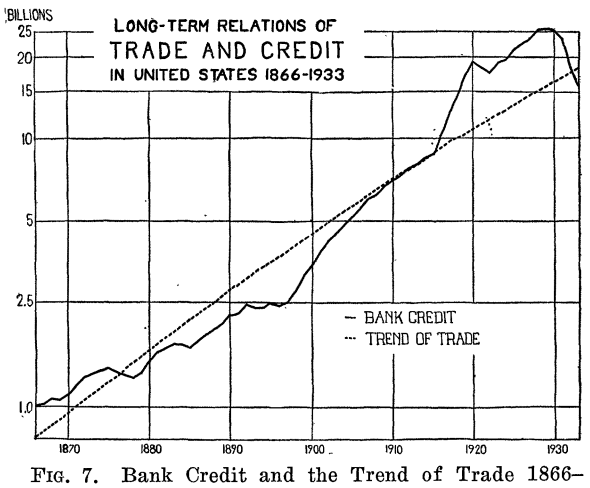

FIG. 7. Bank Credit and the Trend of Trade 1866–1933 (after Snyder).

On the index in the figure the 1926 price level was essentially a war level, a level which has been equalled for less than a decade during and after the civil war, for only about 5 years around 1812, and for only a few years around 1780. Possibly the 1926 level of prices has been equalled or surpassed only in some 25 years of the past 180—possibly one seventh of the time. What is the sense of talking about the 1926 level as normal? Can there be any such thing as a normal level? But the answers to these questions are not my main concern. I am trying to show that one important economic index *viz.*, that of wholesale prices, has very large long-term fluctuations. If you should try by any statistical means to forecast prices between 1913 and 1930 from the data for prices 1895 to 1913, you must inevitably fail.

Let me take another index, namely, Carl Snyder's comparison (Fig. 7) of bank credit versus the trend of trade.[10] Bank credit is a medium of exchange and trade is an exchange of goods; they may both be more than just that, it depends on one's definitions. You will note that the line for bank credit was constantly below that for the trend of trade from 1877 to 1907 and constantly above it from 1917 to 1932. I may again warn that other indices by other persons may show differences in detail, possibly considerable differences; but the point is that we have had a major oscillation of the amount of credit about the trend of trade during the past 60 years, that in so far as Snyder's figures are sound (and they are the careful work of one who tries honestly to follow the statistical situation) we have had for about 15 years a great excess of bank credit over trade; that excess has now perhaps been liquidated. It is barely possible that it had to be liquidated. We are no longer adherents to the ancient motto, "Neither a lender nor a borrower be," we have departed far from the doctrine that "the borrower is servant to the lender," possibly the lender has become merely the dupe of the borrower; but under any system of folkways and mores in a society which uses credit, it may be that at times we have over-extension of credit. What then happens or what to do about it or what may now happen next I do not know. Perhaps others do not know. Whatever others may do, I do not pretend to know when evidence available to me shows that I have no business to claim to know. The reason we have so many failures in forecasting is that we presume to forecast the as yet unforecastable or attempt to control the as yet uncontrollable. So long as there are among us in high position those who exercise that presumption it may not be wholly amiss that a less competent, a less presumptuous person like myself, be permitted to address you on such a subject as I have to-night.

SCIENTIFIC EVENTS

RECEIPTS OF THE NATIONAL FORESTS

INCOME from the national forests for the fiscal year ending June 30, 1934, amounted to $3,314,691, an increase over 1933 of $688,642 or 25 per cent. Gains in timber and waterpower receipts were offset to some extent by decreases in revenues from grazing permits.

Timber sale receipts amounted to $1,499,216, resulting largely from renewed operations in sales contracted in previous years. National Forest timber

[10] *Economic Forum*, pp. 275–290, summer, 1933. Mr. Snyder has very kindly supplied the data from which to reproduce and bring up to date so much of his Chart II as I need in Fig. 7.